apparently quite different and many miles from their usual habitat. This sporadic appearance of vagrants is well-known to all collectors and has always been a puzzle.

It should be pointed out that the author offers these suggested explanations tentatively, in the hope that critical observations may be made which may serve to confirm or destroy them. He would be glad to receive any reports of relevant observations of behaviour.

REFERENCES

(1) Williams, C. B., Fisher, R. A., and Corbet, A. S. (1943). *J. Anim. Ecol.*, **12**: 42-58.

The large Tortoiseshell Butterfly's Life-cycle

By E. P. Wiltshire, C.B.E., F.R.E.S.

I found Mr. Allan's recent article on *Nymphalis polychloros* L. (Vol. 79, Nos. 6-8) most interesting. It raised two questions about this fascinating butterfly:

1. whether it is fertilised before hibernation in this country;
2. whether it has two broods in the year, or only one.

The first of these was his article's main subject, and on this one can only agree with him that it could be solved one way or the other by dissecting females before and after hibernation but that as the species is now almost extinct in this country, this investigation must probably be left to someone in a part of Europe where it is commoner. Even there its habits make it less easy to investigate than Edland(1) appears to have found the Noctuid *E. transversa* of which he examined many females in order to answer a similar question about it.

On the second question, the number of broods in a year, Mr. Allan quoted Verity and various old authors to the effect that there are two in the South. I am writing this note principally in order to point out that Verity appears to have corrected his opinion towards the end of his life. In *Le Farfalle diurne d-Italia* **4**, 349, Verity states that an important biological character of the genus *Nymphalis* (to which he attributes two Italian species, *antiopa* and *polychloros*) is that it always has only one annual generation. Of *polychloros* in particular (id. p. 356) he says (my translation, slightly shortened):

"It is already known that there is only one annual generation, hatching in June-July; but the disappearance of the imagines shortly after eclosion, their spending the summer in an aestivating lethargy, and their reappearing during the whole autumn has led to the belief in two broods, at least in Southern Europe, which nearly all the authors mention as sure. Failla, indeed, observing three appearances in Sicily, attributed them to three generations. In Florence they disappear for the second time to hibernate

(1) Torgeir Edland, 1965-6: "Reproductivity of *Eupsilia transversa* (*Norsk. Ent. Tds.*, **13**: 4, 903). He summarised as follows:—"Of 30 or 40 ♀♀ trapped during autumn of 1963 and 1964, none contained any spermatophores, while 72 ♀♀ trapped in the spring contained as many as a mean 3·4 spermatophores. According to (various authors) each spermatophore may be assumed to represent one successful mating".

from December to the end of February and during this period I have never seen it as *V. atalanta* is seen. But from the 15th (February) they begin to show themselves as *atalanta* does, but more gregariously, because they usually form groups for the purpose of hibernation, attaching themselves to old trunks but preferring sheds, cellars and empty holes in general, and entering houses and settling on the ceilings and rafters. In the spring they quickly wear themselves out and die earlier than *atalanta* and *antiopa*, disappearing at the end of April".

Of *Inachis io* (id. pp. 359 and 361) Verity declares that it has a similar phenology, but not *Aglais urticae*.

It is remarkable that Verity gives details of the hibernation places but not of the aestivation places. I wonder whether modern and experienced Continental lepidopterists would agree with Verity's very definite statements quoted above.

Tutt's statement, which Mr. Allan quoted, is in contradiction with Verity's conclusions, for Tutt claims in one year to have bred two generations both of *io* and *polychloros*.

Perhaps one can explain this contradiction by regarding Tutt's breeding successes as exceptional, and Verity's conclusions as referring to the normal phenology. The mystery, however, could surely be decided by marking and releasing newly hatched midsummer *polychloros* (and *io*) adults and endeavouring to recapture the marked examples again and again over the ensuing months. I hope this article may encourage someone in a suitable country to make the experiments. For *io*, Britain would seem suitable.

Verity, *loc. cit.*, does not touch on the question of the time of mating and this was doubtless because he did not know the answer to Mr Allan's first question. In this connection, on p. 155 of his article, Mr. Allan asks why and how the *receptaculum seminis* evolved if not for the purpose of storing the spermatozoa "during a period unfavourable to fertilisation." The organ of course exists, as far as I know, in virtually all lepidoptera, irrespective of the period that elapses between impregnation and oviposition. According to the figures of Bourgogne[2] the organ is found in the primitive Monotrysians (Micropterygidae, Eriocraniidae), the intermediate Exoporians (some Hepialidae) and the more highly evolved Ditrysians (Nymphalidae and other families). One wonders, of course, whether the *receptacula seminis* of species with a short imaginal life are less developed than those of species whose oviposition is delayed or prolonged, for the latter would appear to require the regulating function of the organ more than the former. An example of a slightly delayed and considerably prolonged oviposition, without either aestivation or hibernation, is afforded by *Aricia allous* as observed by Hoegh Gulberg[3], who found that its bred females started ovipositing four days after impregnation and continued until death 53 days later. A third phenological category is that of the female migrants which start their migrations with undeveloped ovaries. If these are mated before the migration starts, the *receptaculum seminis* would seem to have more important work to do than if mating occurs on arrival at the breeding ground.

[2] J. Bourgogne, 1946. Un type nouveau d'appareil génital femelle chez les lépidopteres. (*Ann. Soc. Ent. de France*, CXV, 69-79.)

[3] Ove Hoegh Gulberg, 1966. North European groups of *Aricia allous* G.-Hbn. Their variability and relationship to *A. agestis* Schiff.

Mr, Allan's article, indeed, suggests that there are in all these cases numerous fields for research to be pursued both in the field and laboratory. While waiting hopefully for the results of such investigations, one would like to know if Verity's later conclusions about the life-cycle of *N. polychloros* and *I. io* are indeed accepted.

I have read Mr. Wiltshire's paper with much interest and shall be not a little pleased if it leads to research "both in the field and in the laboratory." It has seemed to me for a good many years that the subject of my article (fecundation before hibernation in the case of *N. polychloros* L.) is one of those matters upon which scientific research by trained laboratory workers is much overdue. I know of nobody at our universtities who is undertaking this work at present. It is to be hoped that Mr. Wiltshire's excellent paper will now stimulate some to undertake this task. Our field workers of course can do much to help in elucidating and interpreting bionomics — a sadly neglected branch of entomology.

One would like to see so many more papers in the *Record* devoted not merely to accounts of breeding moths and butterflies from egg to pupa but to the actual *habits* of the living insect in its larval, pupal and imaginal stages. Novel and unexpected traits in behaviour often occur when observing the habits of the living insect. Some years ago Mr. Wiltshire gave me two pupae of the spurge hawkmoth (*Deilephila* (*Celerio*) *euphorbiae* L.), and the behaviour of these very active pupae prior to eclosion displayed a quite unexpected and extremely interesting series of events, which were duly recorded in this magazine (Vol. 64 (February 1952), pp. 37-40).—P. B. M. Allan.

Three weeks in Cyprus, April-May 1796

By C. G. M. de Worms, M.A., Ph.D., F.R.E.S.

Cyprus had for a long time been on my programme for a spring visit and it seemed the obvious choice for 1967, since not only had the political situation so greatly improved, but it was one of the few places not too far disatnt which was in the sterling area and therefore did not come within the purview of the restricted travel allowance But as usual, though its lepidoptera had been very thoroughly studied over a long period by a good many eminent collectors, in particular by H. Rebel (1939, *Mitt. Münch. ent. Ges.*, **29**: 487), apparently the only account of collecting in recent years is by Mr R. F. Bretherton who spent a very profitable week in that island in 1954 (*Entomologist* **87**: 207). The end of April appeared to be the most propitious period for obtaining the earlier species of butterflies, but I had heard from Mr C. W. Mackworth Praed, who was in Cyprus at the beginning of that month, what a very late and almost record cold spring had been prevailing there.

I left London by air at midday on April 24 and after a brief stop at Athens we touched down as it was dusking at Nicosia airport where I picked up a new Vauxhall Viva which was to serve me very well. I